apparatus, in which he tried several of his clocks for weeks together, in a vacuum as complete as an ordinary air-pump can produce. It consisted of a pear-shaped copper vessel about 30 inches diameter on a level with the bob of the pendulum; a strong ground brass plate was soldered to its top, on which the clock was fixed; and this was covered by a bell glass with a ground edge, like the receiver of an air-pump. A narrow slip of plate-glass below permitted the reading of the arc of vibration, but as this had a very small surface there was no danger of its fracture.*

I wish to direct the attention of astronomers to page 227 of Sir Edward Sabine's memoir, already referred to, where he describes the retardation in an atmosphere of dry hydrogen at the normal pressure. It is such as would occur in air at a pressure of 5·70 inches. A clock in such an atmosphere would require far less maintaining power, and would therefore work with less friction; while the oil applied to its moving parts would not be liable to chemical change from absorption of oxygen. There also would be much less danger of any alteration of the medium by leakage than in the case of a partial vacuum. Mr. Carrington's coefficient of barometric retardation is higher than what I found for the Armagh clock's mercurial pendulum = 0·37, or Mr. Baily's result for a detached one (*Phil. Trans.* 1832, p. 436) = 0·41.

The difference probably depends on the arc of vibration. This varied but little during my observations, and was allowed for in Mr. Baily's.

It may interest some readers of the *Monthly Notices* to know that the first application of a vacuum to horology was made about sixty years ago by Manton, the celebrated gunsmith, who submitted to the Admiralty a chronometer enclosed in an exhausted receiver, and wound through a stuffing-box. It was tried during a voyage of two years by the late Admiral Beaufort, who reported very favourably of its performance: but the Admiralty took no further action in the matter, and the affair seems to have been forgotten.

Observatory, Armagh,
December 20, 1872.

On a Compensation for the Barometric Errors of Clocks. By E. B. Denison, LL.D., Q.C.

Mr. Carrington's paper in the November *Monthly Notices*, on the retardation of a pendulum by increased density of air, suggests the expediency of providing a compensation for it, which is so easy that I wonder it has not been done before. It seems indeed from Dr. Robinson's paper just now read, that it has been done in some way; but as he speaks of the difficulty of

* It is to be regretted that these experiments have not been published; they form part of an extensive investigation into the various circumstances which nfluence the going of a clock, which was conducted with great care.

calculating the adjustments for it, I infer that it was different from that which I propose, as that can be very easily calculated and applied to existing pendulums.

But it is necessary to observe that the amount of this error varies largely between clocks of nominally the same kind, and much more between those of different kinds; so much that it cannot safely be assumed beforehand for any clock, but must be determined by experiment. The subject was adverted to, but no experimental conclusion arrived at, by our first President, in an elaborate paper chiefly on Compensation for Temperature, in vol. i. of the *Memoirs;* and more fully and with more definite results by the late Mr. M. Bloxam in an equally elaborate paper on Escapements, in vol. xxii., followed by a posthumous supplement in vol. xxvii. He found this error to be as large as 1^s a day for 1 inch of barometer in a dead escapement clock of his own, while Mr. Carrington's result in a clock of that kind was $0^s \cdot 72$. Bloxam's object was to show both mathematically and practically the inferiority of dead escapements to gravity ones, the contrary of which had long been supposed to be proved by the Astronomer Royal in vol. iii. of the *Cambridge Philosophical Transactions;* and he did show, among other things, that the barometric error ought to be and was much less in his own gravity escapement than in a dead one, and in fact only $0^s \cdot 3$ (by which I mean per day and per inch of barometer throughout).

That escapement, though it was successful in his hands, was too delicate for common use, and I believe has never been copied with success. Hardy's gravity escapement has also quite gone out of use, partly for the same reason. I have not the means of ascertaining the amount of this error in any of my gravity escapement clocks, except as after-mentioned; nor in the detached escapement also described in my book on clocks. But Sir G. B. Airy tells me that the "barometric error of his normal sidereal clock with detached (chronometric) escapement, kept in a very even temperature, is $0^s \cdot 3$," the same as Bloxam's.

It may save trouble to say shortly that the cause of the inferiority of dead escapements on this point (apart from others) is that the expression for their daily gain, $-\delta T$, commonly called "rate," contains a term, which with the best possible construction is probably never less than $\frac{4 \delta \alpha}{\alpha}$ seconds, and generally much more; while the corresponding term for the gravity escapement, or a detached one properly constructed, is insignificant for any such $\delta\alpha$ as can occur naturally. In the Westminster clock, with the double three-legged gravity escapement, it could not exceed $\cdot 02^s$ (printed by mistake $\frac{1}{2}^s$, at p. 117 of my book, where the same general results as Bloxam's are arrived at by a shorter method), nor could it reach $\cdot 05^s$ in any well made gravity escapement. It should be observed too, that the sign of this δT is $+$ in the gravity escapement where it is $-$ in the dead, or is in the same direction as the circular error. In the detached it can be made so too, though it probably never is, by making more of the

impulse before than after zero. Moreover, any variation of arc alters the effect of friction on the dead escapement in a way that defies calculation, and is even opposite in different states of the clock. Increased density of air retards all clocks alike, by diminishing the specific gravity of the pendulum without diminishing the moment of inertia, and also by direct resistance to its motion, which reduces the arc and the velocity more in the descent of the pendulum than the ascent, in which a current has been established. On the other hand, this receives an uncertain amount of counteraction from the circular error, 10800 $\alpha\,\delta\,\alpha$ sec. — its reduction by the pendulum spring.

The exact amount of the barometric error however is not material for the present purpose. We need only find a compensation for it on some assumption of its amount and of the weight of the pendulum, and it will be easily modified for any other. The obvious mode of doing it is to attach a barometer to the pendulum which will raise enough mercury, from somewhere near the bottom to about 30 inches higher, to accelerate the pendulum as much as it is otherwise retarded by the increased pressure of air.

Let $M\,l$ be the weight $\times$ (simple) length of the pendulum, $2\,x$ the width of the tube, and r the absolute rise of the ☿ (both in inches); ϱ its specific gravity, which makes two cubic inches of ☿ very nearly 1 lb.; b the height of the lower surface above O the centre of oscillation, and d the depth of the upper surface below the pendulum top. Then reckoning M in lbs., and omitting higher powers of the very small quantities, we shall have,—

$$-\,\delta\,\mathrm{T} = \frac{21600\,\pi\,r\,x^2}{\mathrm{M}}\;\frac{d\,l + b^2 - b\,l - d^2}{l^2}\ \text{sec.}$$

Since d must be substantially $> b$ for this to keep its sign and be tolerably constant, there is not room to make the jar of ☿ for temperature compensation serve also for the barometer basin in a 39-inch pendulum, unless the rod is of glass, which might then be the tube. But that construction is not so desirable as it looks, since it would allow no adjustment or previous trial of that same pendulum, and would require a very wide jar to make the pendulum as heavy as is now agreed to be expedient. So we may confine our attention to steel pendulums, for which also cast iron jars are the best; and these must come 5 inches above O, as shown in my Appendix, in correction of a long-received miscalculation of Baily's in the above-mentioned paper; who strangely forgot the weight of the jar, and so misled some eminent clockmakers into erroneous notions about the compensation required for the pendulum spring. This mistake escaped Mr. Bloxam's detection, beyond finding by experience that the pendulum which had been made for him would not hold ☿ enough for complete compensation. Dead escapements have a counteracting but uncertain error, which had prevented this from being discovered long before. Hardy did calculate his compensation rightly, but Baily's authority had been accepted as conclusive.

The barometer tube then must either dip into the jar and come up again, which would prevent it from being turned for ordinary regulation, or else must bend over it and go down the right or left side to anywhere between the middle and the bottom, and return the same way. It had better be brought up to the rod again, and the two branches be joined there by heat, and tied to the rod by waxed thread, which will be better than any metal fastening.

A rise of 1 inch of barometer will then be an absolute rise in the long leg of $\frac{1}{2}$ inch ($=r$); and if we make $b=0$ for simplicity, and $\therefore d=9$ inches, and $M=40$ lbs., and put $-\delta T=0^s \cdot 3$, the above equation will give $2x$ practically $= \cdot 03$ inches. If the barometric error is 3 times as much, the width of the tube must be $\sqrt{3}$ times as much, or about ·05 inches, in either case a very small one. I suppose it would be filled with ☿ with both ends open, and then the top hermetically sealed. If the compensation is found to have been overdone, the tube has only to be raised, and if too little it may be lowered, if room has been left for it in the bend.

Longer pendulums are never used in astronomical clocks; but they are in turret or public clocks, which now, with gravity escapements, sometimes exceed the accuracy of most astronomical clocks. If they are found to have this error in any sensible degree, it will be better to put an annular basin on the top of the bob, enclosing the rod, and wide enough to make the absolute rise of the ☿ in a straight tube sensibly = the relative rise. The most usual pendulum for good turret clocks of moderate size is $1\frac{1}{4}^s$ (61 inches), weighing about 200 lbs. with the compensation tubes of zinc and iron. There b may be 7 inches, and d about 2 feet, and that makes $2x = \cdot 162$ inches if $-\delta T = \cdot 3^s$ as before. Larger clocks have $1\frac{1}{2}^s$ pendulums (88 inches) of about 300 lbs., and here b may be 8 inches and $d = 50$ inches, and that happens to make $2x$ the same as for the lighter and shorter pendulum, because the barometer is in a more effective place. If the compensation is defective or excessive, it is better not to shift the basin, but to substitute a larger or a smaller tube. But this cannot happen if the clock has been properly tried before calculating the size for the tube, and it is made of the calculated size.

A few of the largest and best public clocks have 2^s (13 ft.) compensated pendulums with very heavy bobs. The Westminster pendulum weighs close upon 700 lbs.; but on account of the great thickness of the zinc tube, which has to act as a column $10\frac{1}{2}$ feet long, the bob only reaches 2 inches above O. I find that with $b = 6$ inches, and $\therefore d = 10$ feet, the barometer tube should be just 0·3 inches wide if $-\delta T = 0^s \cdot 3$.

But since making those calculations I have received from the Astronomer Royal the daily rate of the clock for 1872, except Sundays and a few other days; which reports itself to Greenwich daily, but is not controlled from thence by electrical connexion, as I find some persons erroneously suppose; and that throws a new light on the question as to this class of clocks. We must

reject for this purpose three evidently abnormal disturbances in the year, one of which reached 8^s in a week of thunderstorms and disturbances of rate, which I have known so produced in other clocks; and the others, of about 5^s each, came suddenly, I suppose from accidental shaking of the pendulum in putting on or taking off the small weight with which it is brought to time in a few days whenever it is above 2^s wrong, which is very seldom; for the daily variation never reached anything like that amount at any other time. Omitting these, the average daily variation was only $0^s{\cdot}4$, or $0^s{\cdot}2$ on each side of zero. Therefore the least barometric error above mentioned would form a very sensible part of the general errors of the clock. But, comparing the daily rates with the Registrar General's weekly reports of daily readings of the barometer, I cannot make out that any part of the clock-error is barometric. The application of any correction bearing a constant ratio (+ or —) to the variations of barometer rather increases than diminishes the average variation of the clock for two periods of 7 and 8 weeks, which I tried both in winter and summer; nor do I find any error of temperature compensation.

I can only account for this by supposing that the barometric error happens to be just compensated by the circular error which necessarily accompanies it. The arc α is about 2° 30′, which I intentionally made in 1860 more than is usual in astronomical clocks, though, I confess, without foreseeing this result, which seems specially to justify it. It is singular however, that the barometric error of Bloxam's clock was rather more with an arc of 115′ than with 90′, which I think much too small generally.

And if this is so at Westminster, it must be much the same in my own four-legged gravity escapement clock with 1^s pendulum, which is fully described in the Appendix to my book, as being better for small clocks than the double three-legs, which is now used in all the best large clocks. I have lately been observing mine, with a view to this question, by the sound of the first blow of the hour at Westminster, which has a special contrivance for making it fall exactly at the right time by the clock; and the error, if any, has been rather against the barometer in a time of very low pressure.

I do not at all conclude from these one or two instances that the barometric error may be generally assumed to be self-corrected in these gravity escapements; but the point is worth investigating by those who have the means of comparing such clocks directly with the stars.

I have no means of ascertaining the rate of my five-legged detached escapement, except over longish periods, and therefore not the barometric error, which never lasts long in one direction. All I can say is, that it has been going above seven years without being touched, except being put back about a minute a year for constant rate, and the arc has not yet visibly decreased, though

the train is not a particularly good one. I do not know how far the Greenwich clock on the same principle resembles it in construction; but from what I hear of that, and my own experience, I am inclined to think this the best of all escapements for astronomical clocks, which have only to keep themselves going with very little friction of a well made train, if the beat at only alternate seconds is not found inconvenient; and it does not answer for half-second pendulums. It is quite unfit for large clocks without a remontoire to equalise the force of the train; and I know by sad experience that the usual fate of those remontoires is to be removed by the first ignorant man who gets the care of the clock out of the hands of the maker.

Gravity escapements however have one clear advantage, even for astronomical clocks, in being able to bear any extra work involving friction which would be fatal to the accuracy of any direct impulse escapement, such as striking a small bell every minute, which I have seen done by one of them, or making electrical contacts, which affect the pendulum when done by it directly.

In comparing the performance of large and small clocks, it must be borne in mind, on one hand, that a long and heavy pendulum gives large ones a great advantage, because the expressions for all escapement errors contain $\frac{W h}{M l}$ ($W h$ being the daily moving force that reaches the pendulum), and $W h$ does not increase nearly so fast as $M l$. In the Westminster clock I know by trial that this fraction does not exceed $\frac{1}{80}$, even for that large arc, while it is certainly not less than $\frac{1}{30}$ in astronomical clocks with a smaller arc, after allowing a good deal for friction. On the other hand, the mean specific gravity of these iron and zinc pendulums is not above $\frac{2}{3}$ of a good mercurial one, which therefore has a better chance against the barometric error in both ways that the air affects it, and will also swing a larger arc always for the same impulse; and α^2 or α^3 appears in the denominator of all the variations, except the circular one. But most of all we must remember that the Westminster clock has to move something near two tons of hands and counterpoises and wheels at every beat of the pendulum, sometimes with and sometimes against the wind, on four dials of $22\frac{1}{2}$ feet diameter, and through all the variations of friction of a train of cast iron wheels.

I do not profess to estimate the value of these differences, but I happen to have the means of comparing the weekly rate of Westminster with an old one of the Royal Exchange clock, by the same maker, with a compensated pendulum of the same length, and a jewelled dead escapement and a train remontoire, and dials only 9 feet wide, and cut gun-metal wheels, and every provision then known for making it "the best public clock in the world," as the Astronomer Royal rightly called it in 1845. Yet its average weekly variation for 1851 and 1852, after omitting all the appa-

rently abnormal disturbances, was very nearly 4^s, while that of Westminster, taken in the same way for 1872, was only 1^s. The Exchange rate was materially improved afterwards, by the substitution of a better remontoire, but I have no later record of it, and the clock passed into other hands in 1860; old Mr. Dent's foreman, who had the making and the care of it till then, and has since had that of Westminster, has often told me that it never approached Westminster in steadiness of rate. The average weekly variation of one of his best "regulators" for the same period as the Exchange clock was 2^s; and the rate of his 1851 Exhibition great clock, with pin-wheel dead escapement and the best train remontoire, was reported as being quite equal to that of the regulator fixed beside it. So that we may probably consider the rate of a rightly made large gravity escapement clock twice as good as that of any dead escapement one, large or small, and its barometric error much less, if not altogether neutralised.

On the Re-discovery of Biela's Comet. By M. Klinkerfues.

On comparing the brilliant meteoric shower of November 27th with those of other years, more especially with that of 1805, in which year, to the best of my knowledge, nothing of a similar character was observed, although the circumstances were particularly favourable, I was led to the assumption that in this instance we were in the closest proximity to Biela's Comet. A simple geometric consideration suffices to show that under these circumstances the comet must remain almost stationary in the neighbourhood of the radiant of convergence for a few days immediately following the occurrence of the meteoric shower, and further that there was even a hope of finding the comet itself, provided the intelligence could be at once transmitted to an observatory sufficiently far south. The paths of 80 meteors laid down by our Castellan, Mr. Heidorn, enabled me to determine the radiant point of divergence (R.A $= 26°$; Dec. $= + 37°$) without bias.

On November 30th I sent the following telegram to Madras: "Biela touched Earth November 27th; search near *Theta Centauri.*" This telegram reached Madras by way of Russia in one hour and thirty-five minutes.

Concerning the result Mr. Pogson, under date December 6th, writes to me as follows:

"Nov. 30th, 16^h, the time of comet rising here, I was at my post but hopelessly; clouds and rain gave me no chance. The next morning I had the same bad luck. But on a third trial, a brief blue break about $17\frac{1}{4}^h$ M.T., I found Biela immediately! Only four comparisons in successive minutes could be obtained, in strong morning twilight with an anonymous star; but direct motion of 2·5 seconds of time decided that I had got the comet all right. I noted it: 'Circular, bright, with a decided nucleus but no